

October 14, 1946

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Dear Tracy:

Please pardon the delay in answering your letter of the 1st, requesting haplophase cultures with which to do classroom experiments. The delay was due to the fact that I was in no position to send these strains. In the first place, I do not have them. They are all Lindegren's strains and his connection with Busch makes it difficult to secure them. This unfortunate situation has been hanging undecided for some time and there is nothing I can do about it except to prod Carl to get Anheuser Busch to agree to send out the strains. This I have been doing for some time but without, apparently, much success. I know, for example, that Wings has wanted one of the strains and Anheuser Busch put their foot down because it so happens that that particular strain could be used to develop a commercially important one of value to the Danish baking industry.

Frankly, I do not know what to do about it because it is turning out to be a really embarrassing problem. The only solution that I see is for my laboratory to go into the business of isolating our own strains, and I am hesitant about this because it does take so much effort and time which I believe can be more fruitfully used elsewhere. I may note this. I am trying to develop a galactose strain, with the aid of the Lindegrens, which will do the same thing that the melibiose did, and so far the results look promising. I think I will be able to talk the powers-that-be into letting me use this set of strains in any way I see fit. I have dumped this problem, and others of like kind, into the lap of Carl, who is honestly trying to get clearance from Anheuser Busch. I think the more cases that we have, the more chance of success.

Last summer Ephrussi suggested a complete exchange of yeast strains between our various laboratories, much like *Drosophila* exchange works. But here again, the thing is still stymied because

of the commercial connections. I can send you diploid adaptable strains which will demonstrate all the phenomenon of adaptation per se, because these strains are my own and are the ones on which I have done much of the recent work on the mechanism of adaptation itself. This, of course, would not solve the fundamental problem of demonstrating the genetics of the segregations as a classroom exercise.

Well, I am happy to hear that you acquired your biochemist, and it certainly looks as if you are going to have a good time chasing the killer substance. My own work is going slowly since we are now concerned mainly with the problem of identifying the nature of the stimulator to adaptation with the vague, and probably vain, hope that it may turn out to be the plasmagene. We are also trying to set up experiments to test the capacity of this substance to transfer genetically negative strains into ones possessing specific enzymatic constitution. Here again, of course, we are working in the dark as to requirements for transform ability. However, it seems to me that these two approaches, although certainly not the easiest ones, are at present the most critical for the fundamental problem.

Give my regards to Miller and tell him I am looking forward to seeing him in Washington at the end of this month at that high-powered physics conference.

With best wishes,

Sincerely yours,

S. Spiegelman

ss/b